DRAFT
BLM Science Team Review of Planning Criteria
March 20, 2006

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Background
The Science Team was asked to provide input regarding Chapter 3 of the "Proposed Planning Criteria and State Director Guidance", 2/06 version, and to complete this review by March 17, if possible. This report documents our review. The team focused its review on three questions:

1. Are the analytical questions sufficient to address the purpose and need?
2. Are the assumptions well founded and adequately disclosed?
3. Are the analytical approaches sufficient to answer the analytical question?

The report provides observations, questions and comments at three levels: primary themes and observations, summary comments, and resource-specific comments. Within each resource general comments precede detailed, page-referenced comments. The individual author of each resource-speciifc comment is indicated by a two-letter abbreviation of the author's first and last names.

Caveats
The Science Team offers this input in good faith, and would like to limit interpretation of our findings in several respects consistent with the organization of the Planning Criteria document. First, this report is incomplete. Due to the limited expertise contained within the team we did not review all sections of the planning criteria, nor thoroughly review all analytical assumptions within the resource areas we did evaluate. We picked out the main points for areas where we have expertise. Failure to comment on an analytical procedure does not necessarily indicate support for the procedure described. Second, the level of detail in the descriptions of analytical procedures varies greatly, and for some resources there was not enough detail to adequately respond to the questions we were asked to address. And third, we did not conduct an editorial review. Given these limitations, our review should not be viewed as any form of science certification of these criteria. The role of the Science Team is to provide advice, not to define or monitor achievement of analytical standards. Nevertheless, we feel our comments do highlight areas the BLM could benefit from further thought and documentation, and hope the BLM finds them useful.

Primary themes and observations
The Science Team identified a number of overarching themes and observations that merit particular attention:

- There is a greater need to acknowledge and discuss the potential influence of assumptions on outcomes
- Where there are multiple and conflicting sources of information (e.g., hydrology) there is a greater need to acknowledge and discuss these sources and the degree of convergence among them
- The error surrounding modeled estimates needs acknowledgment and disclosure
- The absence of an apparent method to integrate the influence of disturbance processes (i.e., fire, insects, wind) appears to be a major limitation for the southern portion of the planning area
- The mechanics and assumptions of the harvest scheduling model need much more description, especially concerning how the sustainability requirements will be met
- Integration across resource models and with the harvest scheduling model is not well described
- The hydrology modeling appears quite complex, yet rests on many tenuous assumptions; consider simplifying the approach
- It is unclear if the effects of the alternatives on wildlife populations will be estimated, or if population estimates for listed species are needed
- It is not apparent how the potentially profound effects of climate change will be addressed

Summary comments
CJ - Overall the presentation is very clear and easy to follow. The sections are laid out well, each following the same format, allowing easy access to a wide diversity of information across topics/modules. Given the immense range of Resource and Resource Use that must be included in the planning and assessment of Alternatives and the very short time-lines for the development and assessment of alternatives, the presented material is quite impressive. However, in some areas the depth of material presented seems inadequate and it is not always clear whether this is due to a shortage of time to capture the information for presentation or an inherent lack of depth in the information. In either case, as a reviewer I am left with many questions regarding how and what analyses will be done.

CJ - There seems to be a pervasive feeling of steady state analysis in this Planning Criteria document. Disturbance of habitat and biological units (species and populations) happens naturally, and some argue that such disturbance is a necessary component of community/ecosystem dynamics. Seems naïve, or at least unwise, to score so many aspects of the Alternatives by such small scale (time) changes or resistance to change.

CJ - Where is climate change or at least climate cycles in this process? How will these very large scale types of uncertainty (population growth is another) be included in the final assessment?

CJ - Finally, how do all the units/modules (Fisheries, Hydrology, etc.) interact or conflict with each other? Where is the Ecosystem Management in all of this?

TS - My overall reaction is that while many of the analytical methods that I reviewed appear to be reasonable at general level, the descriptions are too brief to really allow a reviewer to dig into the details behind the general methods that are described. Consequently, most of my comments are questions that one would need answered before stating definitively how well the methods really fit the goals and objectives.

The following appear to be the most significant limitations of the approach:

1. Lack of fire in landscape models—fire will be a key process in the drier part of the area
2. Lack of climate change effects estimates
3. Lack of viability measures or population effects measures. Amounts of habitat for species may not mean much unless there is a way to take into account spatial pattern and dynamics of that habitat and how it might affect population levels. Running the PATCH model might be helpful here for the northern spotted owl.
4. Using a growth and yield model to estimate forest successional change.
5. Many of the measures emphasize late successional habitats and species, for good reason. It is important, however, to identify other ecological issues that may pop up in 100 years that do not seem very important now (e.g. hardwood decline, climate change effects).
6. Nowhere does the analytical approach describe how uncertainty and error will be dealt with in the modeling. This should be discussed explicitly.

While it may not be feasible or practical to address these limitations (e.g. 1-5). It will be important to address how they might influence the results and conclusions in any discussion of the findings.

JH - Would be helpful to have an explicit statement of how each analytical question is intended to address a stated Purpose and Need (i.e., why is this question being analyzed?)

JH - How will the outcomes for all the different resources under all the different time intervals be evaluated across Alternatives? For wildlife alone there are half a dozen species that each have different habitat requirements and thus are each likely to fare differently according to Alternative. How will the metrics for each resource be integrated into decision-making? Also, how will time intervals be evaluated? It doesn't seem reasonable to compare all resources across Alternatives at each time interval, so perhaps the process would be more efficient if fewer intervals were analyzed.

JH - Integration between Ecology and Wildlife sections could be developed, since Ecology represents a coarse-filter approach to maintaining habitat for wildlife.

Vegetation Modeling Overview

SC - The agency suggests that simulation models are better suited to the needs of this project than optimization models (See first paragraph of Constraints and Goals section). If there is a need to address this issue then rethink what is currently written. As it now stands, the discussion

is, at best, misleading. The agency implies that simulation models are better than optimization models because they are "rule based" versus geared towards optimizing the harvest level. This is misleading. An optimization-based model can also be formulated to treat timber harvesting as a residual activity; that is, all other environmental targets and objectives are first satisfied. The statement "These forecasts of effects can be displayed, analyzed, tracked and explained in a more straight-forward manner than with optimization-based models" is also misleading. It is just as easy to take a solution from an optimization model versus a simulation model and track it in a spatial and temporal manner if the needed software is available. Conversely, solutions from simulation models can be incredibly confusing if the software needed to "dump the solution" on the ground is not available. The ease of mapping and explaining the output from a scheduling model is a function of the software available to interpret the solution rather than the solution algorithm itself.

SC - In terms of model formulation, there is a lack of information on an assortment of topics. In order to understand the agency's modeling effort, disclosure of the following is needed.

1. How does OPTIONS work? What is the solution algorithm? How, for example, are the thousands of forest stands sorted and ranked for inclusion in a solution? There are obviously alternative ways to meet constraints and harvest timber. How does the algorithm deal with these choices – or does it? Could an interested party take your data and constraints, formulate it in their model of choice and find a better solution (i.e., one that meets all constraints but has a higher sustainable harvest level)?
2. What is your process for "ground truthing" solutions from the OPTIONS model(s)? Do you plan on taking the output from the OPTIONS model to the "ground" through an assortment of modeling techniques and, as a result, develop a spatially explicit solution (e.g., solution uniquely defined at the forest stand level)? Will you "ground truth" the 1st decade solution or take a look at multiple decades?
3. How are management objectives that require treating spatially contiguous stands in a similar fashion handled in OPTIONS? How are management requirements that focus on stand juxtaposition handled?
4. How many OPTIONS models are you building? Is there one model for each BLM district?
5. You are calculating a sustainable harvest level. How is sustainability modeled? You need to define what you mean by a sustainable harvest level and then explain how you plan to formulate the model to represent it. During the last round of planning, for example, the USFS tied the concept of sustainable harvest levels to forest regulation. This required the agency to: (1) determine which acres were part of the regulated timber base, (2) develop prescriptions for these acres, (3) determine the long-term sustained yield capacity (LTSYC) for each of these prescriptions, (4) constrain the decade harvest flow to be $\leq$ LTSYC and (5) develop ending inventory controls to insure that at the end of the time period being modeled (e.g.150 years), there was sufficient inventory to harvest at the LTSYC into perpetuity. In addition, acres available for harvest but not part of the regulated timber base (e.g. late-successional and riparian reserves) should not be modeled as contributing to any of the timber sustainability measures. I can not tell from the document how the BLM plans to model any aspect of the sustainability issue.
6. How are you dealing with wildfire?

CJ - This section seemed mainly to serve to introduce one of the overarching modeling products that will be use to compile and situate many if not all the Resource and Resource Use specific assessments. However, from my reading it is not entirely clear how the OPTIONS model will incorporate all of the Resource and Resource Use specific assessments other than to act as a graphical compiler, i.e., map making tool. I am sure that this modeling is more involved than this, but from the material presented it is not possible to tell. In fact, the presentation seems somewhat conflicted with respect to complex models and their details. It explains that models that are too complex to understand are considered "black boxes" and thus cannot be trusted. I disagree; models are "black boxes" independent of their complexity, but strictly dependent on the level of detail presented on their inputs, outputs and internal mechanisms such that they can be independently assessed. Such is the case here – OPTIONS is not all described mechanistically, it is only presented as a powerful analytical framework to apply constraints and goals or targets that reflect something – what does the model do, what are the inherent assumptions of the model, and what are its limitations and sensitivities? Even the simplest model can be a "black box" if its use and presentation do not shine any light on the inside of the box. If the OPTIONS model is to have a central role in the evaluation of Alternatives, I think that it is necessary to present its mechanisms in much more details; the lack of details in this section is a major weakness and where I suspect the critics of any decisions made as a result of the Alternative assessment will focus considerable attention.

DD - Page 40 – BLM and Context Modeling
Given that BLM ownership is ~10% (2.5 million acres of 22 million acres) of the total plan area, how representative is BLM land of the remaining total? Are the assumptions about what vegetation patterns on non-BLM land will be into the future valid?

TS - Questions

1. What is the minimum mapping unit size? This needs to be described because it influences the scale of processes that can be examined.
2. Not clear how the Forest Operations Inventory will be supplemented by the CVS grid? What does supplementation mean here?
3. ORGANON is and empirical growth and yield model. How will the modeling deal with its limitations extrapolating beyond the 80-100 year time frame, the lack of a regeneration module, the limitation on the kinds of stand management and forest types (e.g. hardwoods or novel thinning approaches) that it is calibrated with?
4. How will dead wood dynamics be estimated? Will dead wood, created by ORGANON, be decayed over time? How will fuel dynamics be modeled?
5. What about carbon storage and dynamics?
6. How will logging units be identified spatially in the model?
7. Not having fire/insects/disease in the vegetation model is a major limitation in the drier parts of the area.

Ecology
FS - p. 42. Analytical question #1 about degree of departure from historical conditions. It is crucial to say what time period is used to define "historical." Also, this approach can be applied to the riparian zone as well as upland forests – will it be so applied and used to guide cutting in riparian zones?

FS - p. 42. Analytical assumption for Forest Service lands. Assuming that USFS lands will provide a constant amount and distribution of age classes is not consistent with current harvest practices.

FS - p. 44. Questions for scientists:
1. Are there additional sources to describe HRV? It is not clear what source has been used here. In terms of additional sources, has Fire Regime Condition Class been used? I understand that a northwest Oregon analysis is being written up now (contact Jane Kertis for more information), and compilations of references and in some cases data for fire history studies are available.
2. Regional and landscape targets. I don't know much about this, but do think that it is important to begin to think about it. In our region there has been great attention to old growth in this regard, but young, complex, open habitat may deserve similar attention.

FS - Both Timber/silviculture and Ecology sections seem to assume there are no wild disturbance processes, such as wildfire. What will occur when such disturbances occur? Will salvage logging offset program harvest? The assumption in this regard should be stated.

TS - The title of this section should really be "Landscape Ecology". "Ecology" is far too broad for the approach described here.

TS - Question 1
Analytical assumptions

Landscape patterns—Describing landscapes as patches is necessary for many spatial analysis—the so-called gradient approach is not really an alternative

Private Lands—the assumption that private lands will continue to provide the same amount and distribution of vegetation classes over time will be violated to varying degrees. Vegetation change will happen where land use change occurs, especially around large cities and along the margins of valleys. Our research (CLAMS) also suggests that with increasing intensity of management, hardwood forests will decline further on industrial lands and perhaps on private nonindustrial lands as well. With riparian zone protections under the state forest practices act, vegetation along many larger streams will be expected to change—probably an increase in larger conifers in some areas.

Older forests in large blocks a priority--This is probably valid, at least for the next 10-20 years. Appreciation of the value of other successional stages (e.g. diverse, open canopy and hardwood stages) is increasing however. For a 100 year projection, it will be important to also track these other successional stages. In addition, riparian vegetation is of great interest—I don't see that discussed in this section. It may be discussed under fisheries, but I think disconnecting the riparian and terrestrial components is not a good idea.

Analytical methods

Vegetation Classes-- These seem fine for the landscape characterization. They need to be more quantitatively defined, however. Will the criteria used to classify vegetation patches differ on BLM and non BLM lands? The IVMP data do not have information on stand density or structural legacies. How would that difference affect the analyses?

What assumptions will be made about growth of plantations within reserves on FS lands? In other words, what will you assume about the type and pattern of thinnings on these sites?

Fragstats—you might examine the recent paper by Nonaka and Spies (2005) in Ecological Applications 15(5) 1727-1746 for some information on what landscape metrics to use.

Historical Range of Variation—We have some estimates for the Coast Range. We might be able to provide some estimates for other parts of the BLM landbase with some additional analyses.

Analytical conclusion

Might not want to focus exclusively on older forest—also consider other stages and types such as diverse early successional stages and hardwoods. Try to anticipate other ecological issues that may emerge over time.

Questions for scientists

Other sources for HRV? None exist at this time. We could do some first approximation analyses using our LADS model for other parts of the BLM ownership.

TS - Question 2

The resistance of stands to disturbance is an interesting measure. I've not seen a general relationship that can be implemented across a large number of vegetation types. This metric would have to be adjusted for plant series and perhaps disturbance type. It should be backed up with some literature review.

TS - Question 3

The resilience metric needs more development. Not clear what this means or how it will be measured. It will depend on just what aspect of the landscape you are talking about. For example, spatial patterns may take longer to return to a previous state than other measures. I'm not sure what the significance of patch evenness is here. A stronger justification needs to be made for using this metric.

Question for Scientists

1. using HRV, targets could be set for vegetation classes or landscape metrics. Trends and rates of change relative to these ranges could then be measured.
2. Evenness can be measured any way you want. Its value is limited as a sole measure of landscape structure. Other characteristics such as patch sizes, or juxtaposition might be useful additions to this analysis. One could focus on abundance of particular seral stages and discuss them individually. Also could think in terms of focal patch types.

Additional metrics under a "landscape ecology" section could include some wildlife community measures, e.g. groups, focal species or species diversity measures. The reporting of vegetation classes may beg the question of how wildlife communities or species change under the different options? The focus on just listed or a few sensitive species is not up to recognized conservation assessment standards that argue for using a wider range of ecological measures.

CJ - One of the five underlying assumptions for this module is that private lands will continue to provide the same amount and distribution of vegetation classes over time. While I realize that BLM does not have the time nor resources to extend the Alternatives assessment to all land interspersed and surrounding its Western Oregon holdings, it does seem like a poor choice to assume that private land use and cover will remain static across the planning area, especially given the timeframes under consideration (hundreds of years). While the actual pattern and distribution of private land use activities might be difficult to project, a steady conversion of forest and agricultural lands to urban (light and dense) uses is inevitable. Why not use some of the western Oregon population projection work (e.g., USFS-CLAMS, and Willamette Atlas project) as a basis?

CJ - One of the analytical conclusions of the Ecology section will be to rank alternatives by four forest cover patterns, however it is not at all clear what these four metrics mean with respect to forest ecology, or to the ecology of forest resident species. The section goes on to pose a question to scientists regarding targets for patch abundance and diversity, but I think a more basic question has to be asked, or at least stated as an assumption: the large scale metrics of classes and patches mean something biologically as described (species content, spatial scale).

CJ - The second analytical question addresses resistance to disturbance without really addressing the ecological concept of disturbance and resilience. While from a management perspective, static land cover, or static land cover trajectories, may be a useful model for planning, it is not a very realistic version of the landscape. Disturbance happens naturally and the ecology of individuals, populations, communities and ecosystems reflect this. It seems that a much more dynamic view of the landscape is warranted if a 'natural' landscape is to be part of the management objectives. For example, analytical question #3 asks how much can the landscape change and continue to provide key functions? We know that the 'landscape' can go from mature stands to moon-scapes in minutes, but then start back on a trajectory that will ultimately result in mature stands once again – what is relevant in this case is not the magnitude of the change (question 3), but the spatial and temporal scales of disturbance, and our ability to manage patches of land within this patchwork for predicable timber yields and fish and wildlife habitat. Nothing is presented to address the choice of spatial scales for patches and how the natural or historic range of temporal variability will be incorporated in the analysis or assessment.

DD - Page 42 – Ecology - Analytical Assumptions
Follows from above question – Is it a good assumption that private lands will continue to provide the same amount and distribution of veg classes over time?

DD - Analytical Methods and Techniques
What all is defined as "Non-forest"? Depending on the definition and what is done with this "veg class" may or may not be an important element. Are those patches defined as non-forest removed from further consideration?

DD - Data Display - This question may apply to more than this section – data will be displayed in bar graphs, flow diagrams and maps for many of the resources. Will any of the data presented have associated error estimates? Every model should be able to provide error or confidence intervals. Without some sense of the error associated with any reported number comparison across alternatives will be limited. A concern would be a situation where a result from a model output is "better" in one alternative than another, but has greater error associated with the number. It wasn't clear to me that this information would be provided.

DD - Page 44 How resistant are stands to disturbance?
What is the definition of disturbance? A number of effects (drought stress, insect attack, windthrow, etc.) are listed, but not clear on disturbance and their risk to these effects.

JH - Question 2 (pp. 44 - 45)

Clarification is needed on the relationship between resistance to disturbance and habitat quality (if that is the purpose and need being addressed?): what is the underlying assumption?

The assumptions seem too general, since susceptibility to disturbance depends on many variables besides stand age and density (e.g., type of disturbance, forest type, aspect, slope, etc.) Maybe categorizing susceptibility by region according to forest type and prevalent (most threatening) disturbance would refine this approach?

Need more explanation/justification of why young stands are categorized as having least resistance to change. Particularly for the stand establishment phase, it could be argued that structure is unlikely to change much following a disturbance because there isn't much structure to change. On the other hand, very old stands, with very old trees, may be less resistant to structural change because trees are susceptible to breakage and wind throw.

An apparent contradiction: Lower density stands are categorized as more resistant to disturbance, yet they are also more likely to have dense understories which decreases resistance to disturbance. How do these two factors balance each other?

JH - Question 3 (pp. 45-46)

First Analytical Assumption needs clarification: what are the important variables that might indicate recovery: Species composition? Stand structure?

An assumption that is implied but not stated is that evenness of patch distribution represents resilience. A brief explanation of the basis for this assumption would be helpful.

Social/Economic Criteria
SC - The amount of volume estimated in any given decade from the modeling effort is what the agency can offer - it is not what is actually harvested. Over the past decade, we have learned that the amount we offer is often less than the amount that is awarded and ultimately harvested. One reason for this difference is litigation. If all types of volume and harvesting practices were equal in terms of their likelihood of being tied up in litigation, then this difference between offer volume and harvest volume would be a mute point. We know, however, that this is not the case. The location of the volume, the stand structure involved, and the harvest method employed all contribute to different risk factors. These risk factors should be considered when calculating the cost to the agency (Analysis Question #2) and should also be evaluated in terms of their impact on the predictability of the timber supply when Analysis Question #1 is addressed.

Timber and Silviculture
SC - One product from Analytical Question #1 is a graph of standing volume over time by district. I would suggest that this type of graph be provided by land allocation…specifically LSRs and RRs. Even though these land allocations are not part of the design of Strategy #3 and Strategy #4, the agency needs to show the effects of the alternatives on these reserves in order to disclose to the public changes to the NWFP.

SC - If the "annual productive capacity" measures long term sustained yield capacity, then clarity is needed in terms of exactly how this is calculated. The document says, "Timber Productivity Capability Classification (TPCC) will be used in determining the lands capable of supporting the production of forest products on a sustained yield basis." This classification should not be the sole criterion for determining sustainable forest lands. The prescriptions need to be used in concert with this classification to calculate the long term sustained yield capacity. It also appears as though volume is the only metric that will be used. The number and location of acres used in calculating the long term sustained yield capacity is equally important.

SC - If the concept of sustainable harvest levels is based on the premise of forest regulation, then the agency needs to partition their timber volume and acreage estimates in order to distinguish between that portion of the harvest that is not sustainable and that portion that is sustainable. Thinning in LSRs, for example, is not sustainable from the standpoint of forest regulation. The agency needs to define what they mean by sustainable harvest, disclose how their model is formulated to make this calculation, and develop metrics to measure attainment.

SC - Under Analytical Question #3, the following assumption is made: An amount of timber equal to the Annual Productive Capacity will be offered each year. If the APC is equivalent to the LTSYC, then this assumption raises some questions. If any of your harvest schedules have a "deficit" structure (see Davis and Johnson, Forest Management, Chapter 16), then this assumption is impossible to meet unless harvest from lands outside the regulated timber base make up the difference between what the existing stands in the timber base can contribute and the LTSYC. If any of your harvest schedules have a "surplus" structure (see Davis and Johnson, Forest Management, Chapter 16), then you can't offer any volume from lands outside the regulated timber base (e.g. LSRs and RRs) unless you offer it in place of volume emanating from

the regulated base. Once again, the agency needs to clarify the entire sustainability component (definition, formulation, and metrics) and partition the volume and acre measures accordingly.

TS - Questions

- Include losses of the private timberland base to land use change?
- How will coarse woody debris be modeled?
- How will variable density thinning and other federal thinning approaches be modeled?
- How will density-independent mortality in older forests be modeled?
- Can carbon storage be modeled as well?

Special status species—Plants and fungi

TS - Question 1 How will each activity affect habitat…?

TS - Not clear to me why species endemic to conifer stands "will not receive the full array of conservation measures" (analytical assumption 5). Is this a technical/scientific decision or a management priorities decision? This should be clarified.

TS - The statement is made (bullet 7) that "some management activities…can achieve desired habitat change". What is the scientific evidence to support this claim? Without more information, this seems like wishful thinking.

TS - In bullet 9, it states that pre-project surveys for fungi will not be conducted. Again, the rationale for this should be clarified. It is also stated that as a result there is a "higher" degree of risk…with fungal species. Why is the risk to these species higher if pre-project surveys are not done? A pre-project survey only collects information—it is not an action that puts fungi at risk. It is clear that uncertainty increases without a pre-project survey. To really assess risk one would need to know the relationship between forest management and affects on fungal species. Not clear to me that you know enough about the relationships to quantify risk.

TS - Bullet 12 states that the regional database (Geobob) is likely to "overstate" the actual number of occurrences. Why do you conclude that the error would be an overestimate? It could also be an underestimate, if the historical sampling was spotty or biased toward certain types. I don't see any information in this assumption to support a statement of direction in the error of the estimate of actual population size. Without more supporting information all you can really say is that the size of the actual populations is not known.

TS - Table examples

These tables would be a useful contribution. They lack any characterization of the certainty of the information, however. The absence of discussion of data quality and uncertainty is a major shortcoming that is particularly important with rare species. It might be useful to add a column that describes the certainty of the information. It might be also helpful to add a "unknown" descriptor to the list of responses under the "Habitat Quality Change:" column. "Neutral" is not the same as "unknown". This issue of certainty is particularly important when describing effects on groups at 25, 50 and 100 years.

TS - Question 2 How does each alternative affect habitat and how will ..plant and fungi functional groups respond?
The "functional" groups in this example are described in terms of forest and landscape structure (e.g. edge, mixed hardwoods). These are not really "functional" descriptors in a strict sense. They are vegetation/landscape types. The word "functional" implies a level of understanding of the role of these rare species in ecosystem processes that has not be described so far in this document and is probably not justified given our understanding of the autecology and ecosystem ecology of these species. Either increase the description of functional groups to justify use of this term or use another term such as "habitat groups" or "species groups".

Wildlife
JH - General - Need clarification on the metric that will be compared among Alternatives: does "levels" mean "acres of suitable habitat"?

JH - Question 1 (Spotted Owls pp. 73-74)
Analytical Assumptions

Will a Habitat Suitability model be used to assign HSI values to landscape units? Could HSI models and protocols that have already been developed be applied to avoid re-inventing the wheel?

Does the assumption of no change in critical habitat through time refer to habitat boundaries, or habitat structure within boundaries? If the latter, and explanation or justification is needed. (Ditto for Marbled Murrelet, p. 74)

Analytical Conclusion (p.73)

It is not clear if there is a single metric that will be compared across Alternatives. If so, what is it: acres of suitable habitat? (Ditto for Marbled Murrelet and Sage Grouse)

How will patch size and connectivity be assessed? Landscape pattern is an important consideration for Spotted Owl habitat, especially if Barred Owl influence is to be factored in to the analysis. However, the factoring in of variables mentioned at the bottom of p. 73 will complicate the analysis immensely. How will this be accomplished? It might be possible to use a natural history simulator to evaluate likely expansion areas for Barred Owls and consequent competition with Spotted Owls.

Data Needs (p. 74)

Does BioMapper actually derive habitat relationships? What does BioMapper do and why is it the best tool?

Fifth-field watershed maps are listed as a data need, but on p. 73, it is stated that dispersal habitat will be quantified by sixth-field watershed.

JH - Question 4 (Bald Eagle, p. 77)
Analytical Assumptions

An inverse relationship between habitat quality and distance to disturbance would imply that the shorter the distance, the better the habitat. I don't think this is correct; rather quality is positively related to distance to disturbance.

JH - Question 5 (Special Status Species pp. 78-80)

Analytical Assumptions

How will "conditions of wildlife species" be measured? Perhaps an underlying assumption for all wildlife species that should be explicitly stated is that providing adequate habitat is assumed to maintain viable populations.

Assessment Methods and Techniques

What criteria will be used to identify representative species from groups with similar habitat associations?

Need more explanation of what "watershed indices" are and how they will be used to evaluate Alternatives.

JH - Question 6 (Neotropical Birds pp. 78-80)

Why not include all songbirds, or even landbirds, in this group?

Analytical Assumptions

More context is needed for the assumption about managing for ecosystems rather than single species --- how will this assumption affect analyses?

More clarification needed on the "conservation strategies" and "habitat groups" referred to in the last assumption. Is this an assumption or a method?

Assessment Methods and Techniques

Some species will be left out where data is insufficient to describe habitat attributes suggested by PIF conservation strategies. A list of these species should be documented.

More clarification needed on what biological objectives will be used, and how they will be used to assess trends in habitat, and how they will be used by decision-makers to evaluate alternatives.

JH - Question 7 (Elk pp. 83-84)

Assessment Methods

More explanation and justification needed for assignment of Habitat Effectiveness score of 0.4 for certain watersheds. What does "viable" mean in this context? How is it measured?

Analytical Conclusions

The metric to be used for comparison is unclear (acres where HE >/= min.?)

The scale of comparison among alternatives is also unclear; it doesn't seem practical to compare at the scale of 5th field watersheds; a summary metric for each alternative would make for easier comparisons.

JH - Question 7 (Fisher p. 84)

Habitat information is available and models have been developed for fisher that would apply to Oregon forests:

TS - Will the value of diverse early successional conditions within the territories of NSO be included in the models? In other words, will the models include landscape effects that have been shown to contribute to the fitness of owls in the southern parts of the range?

TS - Elk
Talk to a elk biologist/researcher. Most of them do not put much stock in the Wisdom model, especially for the cover component. The opinion seems to be that forage quantity and quality is most important. How do you intend to project forage patterns and dynamics?

TS - Fisher
Clams might be able to help with the fisher model

Fisheries
FS - p. 85+. Fisheries. What should be the distribution of effort here? We have 6 p. on big wood and the entire content concerns wood, water temperature, and fine sediment. Why only these topics? Are they taken as useful surrogates for lots of other habitat issues that could be considered? Hey, I love big wood in streams, but this seems like too much, unless a good (even one sentence) case is made from for the surrogate role of this system component. Questions about the big wood story: 1. only one size is used (one size doesn't fit all) – small streams may get plenty of benefit from pieces appreciably shorter. 2. I am curious to see how the wood modeling is set up – especially how the role of debris flows is considered across the diverse geology and physiography of the BLM lands.

FS - Display of findings in tables of numbers of watershed in an out of compliance with objectives – I take these tables to be examples of how results of analysis will be displayed, right? That is not entirely clear. I am wondering how to interpret and use this information once real values are plugged into the table. Isn't there a way to show degree of compliance rather than yes/no, so that one could see if non-compliance might be corrected or is it a lost cause; or if we learn more and decide to change the threshold, we can better see what the consequences would be in terms of compliance?

FS - p. 92. Analytical question #3. Citations are in References section. What is the strength of argument that the change in flow used as a threshold here if critical? The text cites Harr (1992), but he is not a geomorphologist and I wonder if that is a good source for this critical piece of information.

FS - p. 93. Analytical question #4. Sediment delivery seems to concern only fine sediment – is that so? While related, coarse sediment and fine sediment have rather different dynamics.

CJ - As in all of the sections that I read, the assumptions need to be better supported by the scientific literature. If they are going to be advanced as 'statements of fact', then some support is necessary, again to fend off the critics.

CJ - The table of Special Status Fish species lists roughly 14, but the High Intrinsic Potential model assumptions address only two, and the actual analytical methods are applied to only one. What is the relationship between coho high intrinsic potential and all other species of interest? How is any planning based on coho specific habitat descriptors going to affect (positively or negatively) the other fish species? I understand that the detailed habitat requirements may not be known for all fish species, but would it be possible to develop a 'translation' between coho IP and other species, even if only qualitative?

CJ - The interface between Alternatives and Fish Habitat seems to be miles of stream impacted per 5th field HUC. While I agree in general with this approach, is there more than just the absolute amount of habitat, such as pattern, stream order, connectivity, upstream/upslope habitat condition/type, that might be equally if not more important?

CJ - In step 5 of the Analytical Method treatment of stream miles with wood is introduced. Where did this come from? How is restoration or mitigation going to contribute to the management strategy? It seems unwise to build a management scenario that requires a 'fix' from the very start.

CJ - Analytical Question #2 addresses stream temperature through stream shade, but it is not clear where the stream shade analysis comes from, or that riparian shade is all that matters from stream temperature – what about hillslope and headwaters (intermittent stream area) shading?

CJ - Analytical Question #3 addresses changes in peak flow, but focuses entirely on 2 – 5 year events. What role do longer period natural and non-natural flood return intervals plan in either our thinking about stream hydrology or fish biology? 2-5 year events seem like small potential signals to the land management planning horizon of 50-100 years.

CJ - Analytical Question #4 addresses sediment delivery to streams and its effect on fish. The focus seems heavily slanted towards salmonids and fine sediments, what about suckers and lamprey, do they have the same response criteria for instream sediment conditions? It seems a bit of a cop-out to just say that predicting sediment delivery to streams is difficult, and that it is hard to predict sediment impact on fish habitat. As a result, sediment impacts are only included in a narrative manner – so does this preclude their incorporation into the OPTIONS model, and thus into the actual assessment process? Other parts of the Resource and Resource Use modules have involved considerable uncertainty but a quantification approach is advanced – I think that it is important to do so more completely here as well.

DD - Large wood delivery – debris flows deliver wood, but the question of frequency of these events is pertinent. Wood is good, but a debris flows by themselves are not just a wood delivery mechanism. Unnaturally, if it can be defined, high frequencies of debris flows should be a metric for evaluating the alternatives.

DD - Large wood is only 24 inches in diameter and 50 feet long? I don't recall all the definitions out there, but this sounds like "key pieces" and other important LWD are other "large" sizes and total volume (ODFW and others have used several metrics).

DD - One reason given for not tracking the changes of smaller trees is that "hardwoods recolonize following the removal of conifers near streams and provide much higher nutrient input to the stream channel". Whether or not nutrients are the issue, not sure I understand the rationale for this explanation, not sure of the linkage to LWD. One of the few times that nutrients are even mentioned so a better discussion may be needed.

DD - Page 91 - Stream temperature and fish
Salmonids are the appropriate beneficial use focus, but other more sensitive cold-water taxa could be considered. Huff et al. (2005) has described a number of cold-water vertebrate taxa for western Oregon that could easily be folded into this question. In particular, Pacific Giant salamander (Hawkins et al. 1986) and Tailed Frog have temperature optima colder than the salmonid species listed and would be a sensitive metric (if not for the Fisheries section than for the Wildlife section).

DD - Page 93 - Analytical Question #4 - Sediment delivery and fish
This section lacks detail as to how the analysis will be done. In addition to temperature, sedimentation is a significant threat to salmonid and other aquatic species and both how the increase in sediment delivery by alternative and how it will be evaluated (what does interpret narratively mean?) is not sufficiently explained. How much sediment delivery will or will not affect fish?

Hydrology
FS - p. 96+. Water quantity. I am not a hydrologist and have no experience with the type of analysis outlined here. I am concerned about the number of assumptions and parameterization to support the modeling effort. These factors make it seem very important to compare modeling results with findings from long-term watershed studies, such as those at Andrews and South Umpqua (Coyote Creek watersheds) Experimental Forests, which will be summarized in one of the State-of-Science reviews for this BLM RMP revision process. The general point is to use the different sources of relevant information (e.g., modeling and experimental watershed studies) to see if findings from different sources are in the same ballpark. The BLM-commissioned, state-of-the-science review on peak flows now underway will be a valuable resource on this point.

FS - p. 102+. Water quality – roads as source of fine sediment. I am concerned that this quantitative analysis will obscure the much more tractable story of change in road sediment production over time. It seems that a short narrative would be a good complement to this number crunching. Isn't the basic story that 1. the BLM landscapes are rather completely roaded; 2. therefore, practically no new roads will be constructed; 3. the intensity of maintenance may go down due to budget constraints (therefore, there will be less exposed soil resulting from ditch cleaning and other practices); 4. some road segments will be modified to reduce future erosion (e.g. fillslope pullback, culvert replacement, etc.). Consequently, the net change in sediment production from roads will likely be a decrease. The significance of the existing sediment production from roads is another matter – how will that be put in context? Will roads differ at all among alternatives – especially over the next decade or so of the planning horizon?

I am concerned about how meaningful the quantitative analysis can be – e.g., is the spatial resolution of data bases good enough to support overlaying of stream and road network maps and analysis in GIS with 200 ft buffers.

FS - p. 107. Analytical question #3 – mass wasting. I believe that the Dan Miller modeling system does not make predictions of change in mass wasting potential due to change in vegetation and presence of roads – is that true? Reference to particular slope stability codes is not meaningful to the reader – please clarify. What does the mention of 10 and 100 years in the table on p. 108 indicate?

DD - Temperature and sediment are surrogates for the protection of the specific beneficial uses of salmon spawning and rearing and other resident aquatic life. While the traditional focus in the NW has been on salmon species, other aquatic community components are important watershed indicators. There is no discussion in this section pertaining to important sensitive indicator species or assemblages that have recently become important elements in assessing stream and watershed condition in this state by both state and federal resource management agencies. Specifically, there is the Index of Biotic Integrity for the aquatic fish and amphibian assemblage (Hughes et al. 2004) and RIVPACS-like predictive models for the macroinvertebrate community (Clarke etal1996, Hawkins et al. 2000). OSU's Freshwater Institute recent report (cite) recommended the use of both of these tools and the Oregon Water Quality Index (Cude 2001). If not in the planning criteria, in the later monitoring component of the RMP these indicators would be appropriate look into.

DD - Analytical Question #1 - run-off and peak flow
Whereas the last question lacked detail, this section has plenty (12 steps versus 1 step). Speaks to the question of consistent level of effort for every question and whether some questions because of their of effort are deemed more important in the planning criteria process.

DD - Analytical Question #2 – Sediment delivery
Is this the sediment analysis that is being referred to in Analytical Question #4 from the Fisheries section? If so it needs to be explicit about that.

DD - Page 109. - Analytical Question #4 - Shade
Shade is a surrogate for temperature but not the only one. Shade works well as a surrogate when solar radiation is the primary factor influencing stream temperature. Other factors, specifically the vegetation type, channel characteristics (including substrate conditions affecting hyporheic flow) could be as important.

DD - Some of the other strategies, other than relying solely on effective shade, have been put forward (Forest Service and BLM NWP Temperature TMDL Implementation Strategies (2005)) and these should be looked at.

DD - Page 110 - The planning criteria "assumes that the secondary shade zone is defined as the outer edge of the primary shade zone to 100 feet". The only supporting empirical data offered is from a 1972 paper (Figure A, Brazier and Brown 1972). This appears reasonable, but is that article the most current and relevant work? Seems to me that more recent work has shown

that other buffer widths could be important. Some of the work that has been done on microclimates could be helpful here? Buffer distances, both in terms of providing shade, as well as other riparian and upslope benefits are important to water quality protection.

DD - Page 111 - The PC states " Forest treatments are assumed to fully meet effective shade and water quality standards within primary and secondary shade zones along streams, lakes, and wetlands when the following criteria are met:" My understanding of the intent of the "following criteria" is to ensure attainment of temperature allocation, and cannot assume that other water quality standards are met.

DD - Page 111 - Primary Shade Zone table
The PC states that the "table will be used to determine the width of the primary shade zone, unless a shade model is used for site specific analysis". In order to clarify the intent of the sentence, the following could be added to the sentence "... to delineate the primary shade zone based on site specific information"

DD - It also begs the question about how different the primary shade zone distances will be when and if the shade model is used. If this is a conservative distance then the rationale should be given.

DD - 80% effective shade - A lot seems to be anchored to this number in terms of protecting stream temperature, and a target of 80% shade provides a substantive amount of shade. However, Figure C implies that smaller streams could have up to 0.5 F increase in temperature over a 1 mile distance if nothing else other than solar radiation is influencing stream temperature**.** It seems reasonable to scale the expectations of shade to some simple stream specific components (order, discharge, channel dimensions, etc.). DEQ's TMDLs generally call for site potential shade. The use of 80% shade target would be consistent if BLM ensures that on both spatial and temporal scope of the management activities do not increase heat load or temperature. Temperature is an important factor for the aquatic communities thus a one size fit all approach should build-in conservative assumptions as a trade off for less vigorous site-specific analysis and consider temporal and spatial cumulative effects.

Fire/fuels management
FS - p. 115+. Fire and fuels. Is there any link with FRCC program to characterize geographic variation across the planning area, which can be used to help fit treatments to the land? FRCC is mentioned under Analytical Question #2, but only in the context of assessing extent and presumed effectiveness of treatments. Consolidate references. There is not mention of seeing fire as an integral part of the ecosystem (some more so than others) and how that perspective might influence suppression, selection of fuels treatments, and restoration considerations.

TS - More detail is needed on how fuel will be modeled

TS - The long lists of assumptions under all three questions really need supporting scientific documentation and qualifications. For example, what is the evidence that the particular fuel treatments that are mentioned can be effective? Or, how would you qualify the statement that “hazardous fuels will continue to increase in unmanaged areas, given the wide range of

environments that are dealt with?" Or, how will FRCC be scaled to the spatial units of this analysis and be applied with the vegetation models? What is the evidence that large fuels increase fire severity? How will tradeoffs between fuel levels and habitat needs be addressed?

TS - The statement that "Intensely burned vegetation does not necessarily correlate to high fire severity" is true but it all depends on how you define severity. Severity is often defined as the amount of vegetation killed. Severity can also be defined in terms of soil effects, but these are harder to measure across large areas. Intensity is usually defined as energy release per unit length of burn front. The discussion in this section seems to use intensity and severity as if they were synonymous, they are not. It would help clarify the text if these terms were defined more precisely at the beginning.

References

Allen, Arthur W. Habitat Suitability Index Models:Fisher. Biological Report. 1983. Washington, DC, U.S. Fish and Wildlife Service.

Aubry, Keith B., Raley, Catherine M., Catton, T. J., and Tomb, Gregory W. Ecological characteristics of fishers in southwestern Oregon, progress report 1 January - 31 December 2000. 2000. Olympia, WA, USDA Forest Service.

Carlos, Carroll, Zielinski, William J., and Noss, Reed F. (1997-2000). Using presence-absence data to build and test spatial habitat models for the fisher in the Klamath region, U.S.A. *Conservation Biology* **13**.

Clarke, R. T., M. T. Furse, J. F. Wright, and D. Moss. 1996. Derivation of a biological quality index for river sites: comparison of the observed with the expected fauna. Journal of Applied Statistics 23:311-332.

Cude, C. G. 2001. Oregon Water Quality Index: A Tool for Evaluating Water Quality Management Effectiveness. Journal of the American Water Resources Association, 37:125-137.

Hawkins, C.P., M.L. Murphy, and N.J. Anderson. 1983. Density of fish and salamanders in relation to riparian canopy and physical habitat in streams of the northwestern United States. Can. J. Fish. Aquatic. Sci. 40(8):1173-1186.

Hawkins, C. P., R. H. Norris, J. N. Hogue, and J. W. Feminella. 2000. Development and evaluation of predictive models for measuring the biological integrity of streams. Ecological Applications 10:1456-1477.

Huff, D. D., S. L. Hubler and A. N. Borisenko. 2005. Using field data to estimate the thermal niche of aquatic vertebrates. North American Journal of Fisheries Management 25:346-360.

Hughes, R.M., S. Howlin, and P.R. Kaufmann. 2004. A Biointegrity index for Coldwater Streams of Western Oregon and Washington. Transactions of the American Fisheries Society. 133:1497-1515.

Lyon, L. J., Aubry, K. B., Zielinski, W. J., Buskirk, S. W., and Ruggiero, L. F. The scientific

basis for conserving forest carnivores: considerations for management. 1994. U.S. Forest Service.

Thomasma, Linda E., Drummer, Thomas D., and Peterson, Rolf O. (1991). Testing the habitat suitability index model for the fisher. *Wildlife Society Bulletin* **19**, 291-297.